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I.—THE HINDU BEAST FABLE IN THE LIGHT OF RECENT STUDIES.

PART II.

(In this article the following abbreviations are used of Pañcatantra versions: Tantr., the Tantrākhyāyika.—Pa. or Pahl., the Pahlavi translation.—Syr., the Old Syriac.—Brh., the (lost) version of the north-western Bṛhatkathā (archetype of Som. and Kṣem.).—Som., Soma-deva.—Kṣem., Kṣemendra.—n-w, the archetype of SP., *ν* and Hit.—SP., Southern Pañcatantra.—*ν*, Nepalese Pañcatantra.—Hit., Hitopadeṣa.—Simpl., *textus simplicior*.—Pūrṇ., Pūrṇabhadra.)

In the first part of this article (A. J. P. XXXVI. 44 ff.) I described the relationship of the different Pañc. versions in so far as it seems to me determinable at present. Most of the facts as therein stated have been worked out and proved by Hertel. I now face the more unwelcome task of discussing some alleged facts of the same sort which I hold that Hertel has failed to prove. This task, however distasteful, seems to me necessary. For Hertel admits not the slightest question of any part of his "genealogical table" ("Stammbaum") of the Pañc. versions (Pañc., p. 426); he regards every part of it as absolutely and irrefutably proved, and draws sweeping and important conclusions from it. And in fact, as he rightly says (Pañc., p. 428), no real counter-argument has as yet been presented. Various scholars have expressed doubts about the "Stammbaum"; no one has seriously tried to refute any of

Hertel's arguments in support of it. It is high time that this be done. For some of the points in question are of prime importance, for the proper judging not only of the relationship of the Pañc. versions, but of Hindu literary tradition as a whole.

The trouble with Hertel's "Stammbaum" seems to me to be that it tries to prove too much. Not content with bringing out the many interesting facts to which I have already referred, it undertakes to go farther and establish things which are quite incapable of proof in the present state of our knowledge. The reputation of all of Hertel's work has suffered, unjustly but quite naturally, on this account. Some scholars, seeing the subjectivity and unreliability of many of his arguments, have been inclined to assume that all his assertions about the Pañcatantra versions rest on equally uncertain grounds.

I have tried to apply the acid test to all of Hertel's "Stammbaum", with a view to removing the bad and making the good (which includes the greater part of the "Stammbaum") all the more certain. I was somewhat surprised at the ease with which, as it seems to me, the separation can be made. When carefully analyzed, there is amazingly little sound evidence for several of Hertel's allegations—considering the comparative certainty of some of his other conclusions.

The three specific points which I shall try to show are unproved, and probably unprovable, are the following. I. The archetype "t", containing alleged corruptions found in all existent versions. II. The archetype "K", from which H. thinks all versions except the Tantr. are descended. III. The archetype "N-W", from which he thinks Pahl., n-w, and Simpl. are descended.

All of these hypothetical archetypes seem to me mythical. I think there is no closer connexion between Pahl., Som., n-w and Simpl. than between any one of them and the Tantr. In fact, I have evidence which at least *tends* to show that Tantr. is related to Pahl., and also to n-w and to Simpl., more closely than any two of these latter versions are related to each other. In my careful study of the verses of Book I, I found very few cases of agreement between n-w, Simpl. and Pahl., or any two of them, as against the Tantr., but a considerable number of

agreements between each one of them and Tantr. as against the others.¹

I. The very first assumption in Hertel's Stammbaum seems to me highly dubious; I mean, his assumption of an early archetype "t", which stood between the Urpañcatantra ("T") and all existing versions, and in which Hertel thinks he can point out certain very definite corruptions. He claims to have done this "in für jeden Philologen einwandfreier Weise" (Pañc., p. 443). I cannot let this statement pass unchallenged. Consider what the claim means: it means that Hertel undertakes to establish definite faults in, and to *emend*, a purely hypothetical archetype of *all* existing Pañcatantra versions—an archetype which (together with its uncorrupted original) must have been lost without trace since before the 6th century A. D., according to his theory. "A large order!" Still, I should not wish to deny the possibility of such a thing. But a careful consideration of Hertel's arguments² leaves me with the impression that the thing is a myth. Certainly it seems to me that these arguments totally fail to prove his point.

They relate to six passages in which Hertel thinks he can show that all existing versions go back to a corrupt original.³

¹ For instance, as to the mere occurrence or non-occurrence of particular verses in Book I. There are 7 vss. found in Tantr. and n-w only, 32 found only in Tantr., n-w and the Jaina recensions (Simpl. Pūrṇ.). There are 10 found in Tantr. and Pahl. only, 22 only in Tantr., Pahl. and the Jaina rec. There are 8 found only in Tantr. and Simpl. (or in Tantr., Simpl. and Pūrṇ.). There are, on the other hand, no verses found exclusively in Pahl. and either or both of the Jaina recensions; none found exclusively in Pahl., n-w and a Jaina recension; not more than 3 (of which 2 are doubtful) found only in Pahl. and n-w; and only 2 found exclusively in n-w and either or both of the Jaina recensions. Do not these facts in themselves make it seem doubtful whether Hertel is right in making Pahl., n-w and Simpl. very intimately related, and only much more remotely connected with Tantr.?

² Tantrākhyāyika, (Einleitung zur) Uebersetzung, I. 34 ff.

³ Incidentally, by what right are all these "corruptions" corrected in Hertel's edition of the Tantrākhyāyika? Since his theory is that the corruptions go back to the *archetype* thereof, why try to make the Tantrākhyāyika text seem better than it really was? Hertel shows no such favor to the Southern Pañcatantra; in his text of it he prints not only all the corruptions of its archetype, but many that were certainly not in it, being found only in manuscripts of the inferior subrecension β.

In case one version or another has the "correct" reading, then he calls it a "glückliche Besserung" (so in his very first instance, to be mentioned in a moment). I am constrained to say that his argumentation on this point seems almost wholly subjective, and of the thinnest sort; its strength is in inverse ratio to the vigor and positiveness with which it is put forward. All six cases concern very trivial matters; generally a slight change in, or omission of, a single word. (Scarcely sufficient evidence, as to mere bulk, to justify such sweeping conclusions!) I cannot here take them all up; two must serve for all. I choose the very first two instances given by Hertel; I presume, from his putting them in that position, that he regards them as at least as strong as any. In this I agree with him; the others are no stronger.

His second instance (which we shall consider first) does not seem, in fact, to show any corruption in the supposed archetype "t" at all, according to Hertel's own statement of the case. His opinion that it does so seems to be merely due to lack of clarity of thought. Hertel maintains that in Tantrākhyāyika II. 87 the "original" reading was *yasyānubandhāt*, which Pūrṇabhadra has, for the Tantr. MSS. *yasyānubandhaḥ*; but that the "original" order of the two half-verses was that of the Tantr. (they are transposed in Pūrṇabhadra).¹ In other words, of these two points on which Tantr. and Pūrṇ. differ in the reading of this verse, H. thinks that the Tantr. preserves the original reading in one, Pūrṇ. in the other. It appears that if Hertel had thought the matter out logically, he would have seen that according to his own statement, there is no single feature of the "original" reading that is not correctly preserved in *some* version; none of the individual corruptions can therefore go back to a corrupt archetype of *all* the versions. The "original" reading must, if his argumentation is otherwise correct, have been found in the intermediate archetype "t" (supposing that there ever was such a thing), as well as in the Urpañcatantra. If

¹ The Southern Pañcatantra has what Hertel considers the corruptions of both Tantr. and Pūrṇ. in this verse. The verse does not occur in Simpl. or in Pahl., which Hertel strangely thinks is a sign that it was corrupt in their original. But both Simpl. and Pahl., especially the latter, omit many verses of the original whose readings are open to no suspicion of corruption.

not, then one or the other of the later versions must have changed back secondarily to the original reading; and H. does not claim, apparently, that this was the case.

Hertel's first instance requires more careful consideration. In Tantr. "A 149" (shortly after the verse II. 32) all Hertel's MSS. read *pratyarthito 'ham bhavatā*, "I have been challenged (or, opposed) by your worship". Hertel emends *pratyarthito* to *pratyāyito*, "I have been convinced (persuaded, made confident) by your worship". The Pahlavi must have read something like "requested" for the word in question, which, as H. says, seems to indicate a Sanskrit *prārthito* or the like, unless the Pahlavi translator took *pratyarthito* (wrongly) in the sense of *prārthito*. In the textus simplicior and in Somadeva the passage is wholly changed.

On the other hand, Pūrṇabhadra has *pratyāyito*. So have recensions α, β and γ of the Southern Pāṇicantra (recension δ, evidently by corruption, *pratyāçvāsito*). In the Nepalese verse-collection the passage, being prose, is not found. The Hitopadeça has *āpyāyito*.

Now Hertel says: *pratyāyito* (which he puts by emendation in Tantr., and believes to have been the Urpāṇicantra reading) is evidently the reading of the Southern Pāṇicantra archetype. "Trotzdem wird sie der Quelle desselben—n—w—abzusprechen sein". Why? Because the Hitopadeça has the reading *āpyāyito*, which "kommt in seinen Schriftzügen den anderen Lesarten so nahe, dass man wird annehmen müssen, es sei aus einer Korruptel¹ (!!) hervorgegangen, die Nārāyaṇa konjecturell besserte". This hypothetical (or imaginary) "Korruptel", Hertel tacitly assumes, must have been in "n—w",² the original of both SP. and Hit., and consequently it must have been changed back again to the right reading in SP. Thus by piling sand on sand Hertel arrives at this con-

¹ Why may it not be itself a "Korruptel", or a deliberate or accidental change of some sort, from *pratyāyito*—which is the reading of SP. (Hit.'s closest relative), and which in its "Schriftzügen" comes closer to *āpyāyito* than the reading of the word found in any other version? Does Hertel mean to maintain that wherever Nārāyaṇa has departed from his original, that original must have been corrupt?

² Why, by the way, if the Hit. must be supposed to go back to a corruption at all, may not that corruption have crept in first in "n—w"², the intermediate stage between "n—w" and Hit.?

clusion: the corrupt reading *pratyarthito* was found in "t", from which all existing Pañc. versions are descended. It was taken over thence into the Tantrākhyāyika, and into the Sanskrit original of the Pahlavi (unless it read *prārthito* instead). This corruption was still found in "n-w", the common original of SP. and Hit. The SP. redactor "happily" corrected it to *pratyāyito* (which the Ur-ur-pañcatantra, back behind "t", once had); and the redactor of the Hit. made an attempt in the same direction, and got as close as *āpyāyito*. Finally, Pūrṇabhadra (whose tradition, according to Hertel, is in no way related to that of SP.) also hit upon the same very "happy correction" of his (necessarily corrupt, though unknown) original, and reads *pratyāyito*.

To me the following theory seems to explain the facts as stated much better. The original reading of the Urpañcatantra was *pratyāyito*; and if there was any such intermediate codex as "t", it likewise read *pratyāyito*. (I agree with Hertel that the sense of the passage requires this.) In the Tantrākhyāyika, or at least in the few MSS. of it we know, this is corrupted to *pratyarthito*. In the original of the Pahlavi it probably became *prārthito* (whether independently of the Tantrākhyāyika corruption, or whether they both go back to a common original—a corrupt intermediate "t¹" of their own, if you like—must be left in abeyance). In the original of SP. and Hit., on the other hand, as well as in the source used by Pūrṇabhadra at this point,¹ the original reading *pratyāyito* was preserved unchanged. The Hit. reading *āpyāyito* is surely much closer "in seinen Schriftzügen" to *pratyāyito* than to *pratyarthito*, and the inference to me seems obvious that it goes back to the former, and was introduced by Nārāyaṇa or by his hypothetical immediate predecessor "n-w²"—whether you call it a "konjektuelle Besserung" or simply a change, deliberate or accidental. Why chase all round Robin Hood's barn and assume an old corruption, only in order to assume again a ("happy" indeed!)

¹ It seems to me quite conceivable that Pūrṇ. got the reading from a MS. of Tantrākhyāyika which antedated the corruption found in all our MSS. The number of MSS. of Tantr. now known is very small, and really smaller than it sounds, since Hertel has shown that some of them are only modern copies of one of the older ones. H. has also shown that Pūrṇ. made extensive use of a Tantr. codex.

correction of that corruption—a restoration of the original reading, and that too in *two independent versions* (SP. and Purn., both of which have the correct reading *pratyāyito*) ?

II. A much more important statement of Hertel's "Stamm-baum" is that all the older versions except the Tantr.—to wit, the Pahl., Bṛh., n-w, and Simpl., with Pūrṇ. except where the latter borrowed from Tantr.—go back to a common archetype, which Hertel calls "K", distinguishing the Tantr. archetype as "Ś". If true, this would obviously be of great importance in judging all variations between the several versions, and in reconstructing the Urpañc. Of course, the only way in which such a proposition could be proved is by showing common *changes* or *corruptions* in all these versions, as against the correct or original readings of Tantr. No amount of agreements in correct and original readings of these "K" versions, as opposed to corruptions in the Tantr., could have any bearing on the question. Furthermore, it must be shown in each case that *the same* change or corruption lies at the bottom of the text of all the "K" versions.

Hertel apparently recognizes this principle, and in fact undertakes to show in a few individual cases (he enumerates only four in his first statement of the case, Tantr. Uebers. I. 28 ff.) that all the "K" versions go back to a single corruption, while Tantr. in each case preserves the original. He thinks that he thereby proves absolutely the reality of his "K" archetype. I cannot agree with him, for two reasons. First, the cases he mentions are individually inconclusive. Secondly, they are too few in number to prove the point, even if they were individually sound.

In the first place, as to the few particular instances in which Hertel thinks the "K" versions go back to a common corruption, while the Tantr. preserves the original reading, I think that they all permit, and some of them demand, different interpretations. They can hardly be said even to *tend* to prove his point.

(a) One of them, to which Hertel pays a great deal of attention, occurs in the story of the Monkey and the Crocodile, the frame-story of Book IV. (Of this story, in all important versions, H. gives an interesting and valuable com-

parative translation, in parallel columns, Tantr. Uebers. I. 70 ff.) Those who do not know the story (which is a very wide-spread one) may easily locate it in any Pañcatantra translation. The point which concerns us here is that in all the versions except the Tantr. the treacherous crocodile is represented as inviting his unsuspecting friend, the monkey, to get on his back and visit *his own house*, meaning by this trick to compass his death.

Now, says Hertel (l. c., p. 89), in the Tantr. the crocodile does not make this "absurd proposal" that the monkey should visit his own house—"das liegt ja im Wasser";¹ instead, he proposes to take the monkey, on his own back, over to a lovely island, where the monkey may enjoy all sorts of sensual delights (Tantr. A 286).

Just in this difference lies the point which Hertel finds so important. For later, when the monkey discovers the crocodile's trick, he is represented (in all versions) as bewailing his own *love of pleasure*, which had enticed him ("in spite of his age") into agreeing to the suggestion of his false friend. Hertel thinks this is inconsistent with the account in the other texts: "in allen (anderen Versionen überlistet) der Śiśumāra (crocodile) den Affen nicht durch die Aussicht auf sinnliche Vergnügungen mit jungen Äffinnen, sondern durch eine Einladung in sein Haus". The "junge Äffinnen", it should be said, appear even in Tantr. only through an emendation of Hertel's; the emendation is, however, a very attractive one, and probably the original contained something of the sort.

But it seems to me that Hertel attaches insufficient importance to the references which even the other ("K") versions contain to sensual delights as a motive for the trip to the island. (Cf. the Pahl. and Simpl. readings parallel with Tantr. A 286, Hertel, l. c., p. 84, 85.) In the Pahl. (and seemingly in Simpl., though its account is compressed) the invitation to visit the crocodile's home is *coupled with* a promise of various sensual (not, indeed, sexual) joys.² So

¹ But in Simpl. and Pahl. the crocodile distinctly tells the monkey that his house is *on the island*. This may have been a lie, but the monkey could hardly be expected to know it. So the proposal is not at all "absurd".

² SP., Som. and Kṣem. have lost this entire paragraph in their very abbreviated versions.

that Hertel's sentence quoted above, while literally true, is unintentionally misleading; the other versions *do* motivate the seduction of the monkey *in part* "durch die Aussicht auf sinnliche Vergnügungen". That the original made it specifically *sexual* pleasures I am inclined to believe with Hertel; this feature of the original is indicated by the later *dénouement* in several versions (including SP.), although at the point we are now considering it does not come out clearly in any (not even in Tantr., unless by the grace of Hertel's emendation).

But this is a matter of minor importance. The fact remains that in all the versions but Tantr. the crocodile invites the monkey *to his own house*, promising as an added inducement various sensual delights on the island where he says his home is. In the Tantr. (A 286) where the crocodile first speaks of the island with its sensual delights, there is indeed nothing said about the crocodile's *home* being there. And in all the conversation between the two, as reported in the Tantr., the crocodile never mentions his house. He merely expresses his sorrow¹ at never having made any return for the favors shown him by the monkey (A 281 f., Hertel, l. c., 81-83).

But note the monkey's strange reply to this expression of regret! I quote the monkey's words (Tantr. A 284, Hertel, l. c. 83): "And as for your saying, 'I have never invited you *to come to my house*, to meet my wife, and to eat from my dish', what difference does that make? Such is the friendship only of common people!"

Now according to the Tantr.'s text as it stands, the crocodile had not said anything of the sort at all! Just therein, according to Hertel, lies the great superiority of the Tantr.'s text over the other—"corrupt"—versions, and the striking proof that all the others go back to a single corrupt archetype "K"!

It seems evident that in his haste and enthusiasm Hertel quite overlooked these significant words of the monkey's. They seem to me to show that the archetype of the Tantr. must have agreed essentially with the other versions. The monkey is represented as quoting the crocodile's very words, which consist precisely in that invitation to his hearth

¹ A frivolous query: Is this the origin of "crocodile tears"?

and home which Hertel thinks cannot have been in the original.

Instead of proving that the other recensions go back to a corrupt archetype, this example proves that the Tantr. is corrupt at this point. Its version is glaringly inconsistent with itself. Somehow or other it has lost from its text the preceding words of the crocodile, containing the suggestion that the monkey come to his house, which alone make the words of the monkey in A 284 intelligible.

The Jātaka versions of the fable (Jāt. 208, 342) say nothing about any invitation to the crocodile's house, and of this Hertel makes much, claiming it as a confirmation of what he takes to be the original form of the story. But in the Jātakas the story is otherwise changed in such a way that it would have been rank absurdity to keep the incident of the invitation. The monkey and the crocodile are not friends, nor even acquaintances, in the Jāt. story, and it would have been absurd—not to say suspicious—if the crocodile had invited the monkey to his house. The Jātakas are characteristically intent on the moral lesson—the dangers of sensuality—and leave out all the rest of the story.

(b) Hertel refers (Tantr. Uebers. I. 31 f.) to the introduction to his SP. (p. LXIII ff.) for another instance which he thinks points to a corrupt archetype ("K") from which all versions except the Tantr. are descended.

This concerns the strophe SP. II. 41 = ν I. 47 = Hit. I. 129 Pet. = Tantr. II. 90 = Pūrṇ. II. 116. This stanza contains two words in which Tantr. differs from the consensus of Pūrṇ. and the descendants of n-w (that is, SP., ν and Hit.). Hertel thinks that the Tantr. reading for both these words is clearly the original, and consequently that the other versions all point to a corrupt archetype.

In the first place, this strophe is not found in Pahl., Som., Kṣem. or Simpl., so that it is surely somewhat rash to say that it proves a corruption in an archetype to which all these versions go back. The most that it could prove would be a corruption in some common archetype of n-w and Pūrṇ., where alone the verse is found (outside of Tantr.). But it does not prove even that.

I will quote the Sanskrit text indicated for the common archetype of n-w and Pūrṇ. and assumed by Hertel as the reading of his postulated "K":

*na svalpam apy adhyavasāyabhīroḥ karoti vijñānavidhir
guṇam hi
andhasya kiṁ hastatalasthito 'pi nivartayaty artham iha
pradīpaḥ.*

There are minor variations which do not concern us; thus Pūrṇ. reads *nivartayed* in d for *nivartayaty*, Hit. and v read *prakāṣayaty*, and most MSS. of SP. *sandarṣayaty*, for the same word; but the best MS. of SP. points to an archetype agreeing with Tantr. as to this word.

Now the Tantr. text agrees almost wholly with the above, but reads *avyavasāyabhīroḥ* in a, and *āndhyam* for *artham* in d. These readings Hertel thinks are original, and he infers that n-w and Pūrṇ. go back to a common corrupt archetype. Decidedly a *nonsequitur*, for H. himself says (Pañc., p. 76) that Pūrṇ. used "several Pañc. versions which are unknown to us" (I should add, "and probably other sources, including oral *geflügelte Wörter*, not attached to any literary work, many of which he incorporated among his verses"). It is, then, quite conceivable that Pūrṇ.'s reading of this stanza may have been taken as a whole from Tantr., or from some source having the Tantr. reading, but Pūrṇ. may have changed these two words into accord with another version of the stanza which he knew from another source. So Pūrṇ. affords very slight evidence as to the supposed archetype "K", and there is left only n-w; the "corruption" then, if it be a corruption, may have come in with n-w.

But is the reading of n-w and Pūrṇ. a corruption? It means: "The acquisition of knowledge does not confer the least advantage (*guṇa*—superior quality, superiority) upon a man who is afraid to take a firm stand (*adhyavasāya*, rendered *utsāha* in some Hindu lexicons, see BR.). If a lamp is placed right in the hands of a blind man, what advantage (*artha*) does it bring to him here?" Every Sanskrit word is here rendered in accordance with well-recognized meanings, and it seems to me to make excellent sense.

The Tantr. version means, according to Hertel: "The acquisition of knowledge does not give the least advantage (*Vorzug*) to a man who is irresolute and timid. Is the blindness of a blind man removed here by a torch, which he holds in his hand?" H.'s translation also makes good sense, though I cannot see that it is any better than the sense yielded by the other reading. The verb *nivartayaty* has to be taken in the sense of "remove", instead of "confer"; strange to say, both are well-attested meanings of the word. H. claims—rightly, it seems—that "remove" is the commoner meaning. If so, it would suggest to my mind not that it was the original meaning in this place, but rather if anything the reverse (the *lectio difficilior* is, other things being equal, more apt to be original). And the reading *āndhyam* for *artham*—with the word *andhasya* in the preceding pada—sounds like a secondary rather than a primary reading. Is not the following at least as likely a theory as Hertel's? The Uṛpañcatantra read in pada d *nivartayaty artham*, "confers advantage". The Tantr. redactor was more familiar with *ni-vartayati* in the sense of "remove"; the word *artham* as written in the Cāradā alphabet is (as H. observes) easily confused with **antham*, which—under the psychological suggestion of the word *andha* in the preceding pada—naturally suggested *āndhyam*, "blindness", as the thing which the lamp would not "remove". So arose Tantr.'s reading. I do not say, Hertel-wise, that this is the only conceivable explanation of the variation in pada d; I do submit that it is at least as likely as H.'s contrary explanation (personally it seems to me much more likely), and therefore that "völlige Sicherheit" can hardly be claimed for Hertel's opinion.

Similarly as to the other variation in this verse, that of pada a, in which Tantr. has *avyavasāyabhīroḥ* for *adhyava*° of the others. Hertel must of course take the Tantr. reading as an adjectival dvandva compound, "irresolute and fearful". He thinks this makes much better sense than "afraid to take a firm stand" or "to make a firm resolve". *Geschmacksache*; if Hertel likes it better, we must allow him his preference. To me the other meaning seems quite as sensible, and it accords better, if anything, with Sanskrit usage. I am suspicious of "adjectival dvandvas" both members of which are

supposed to apply to the same individual.¹ Such forms are in any case not common except with color-adjectives. But let that pass; it too may be called "subjective". What I want to emphasize is that Hertel's purely subjective liking for one variant as against the other can hardly be considered proof that the one was original. Others, of whom I am one, happen to like the other better. Hertel does not deny that the other makes sense. His attempt to make it seem poor sense seems to me unsuccessful.

Hertel points out that the variant is a graphic one; *vya* and *dhya* are closely similar in the Çāradā alphabet. Since he assumes *vya* as the original, he infers that the *dhya* of n-w and Pūrṇ. must go back to an archetype written in Çāradā, though (I believe) none of their existent MSS. are written in that alphabet. This is a theory; it is a *fact* that all our MSS. of the Tantr. are written in Çāradā. It seems that it would be more *naheliegend* to assume that our Tantr. MSS. have corrupted an original *dhya*, written in Çāradā characters, to *vya*.

On this point again I do not claim that the explanation I offer is the only conceivable one. I merely maintain that it is quite as likely to be true as Hertel's, and therefore that Hertel is deceived in thinking that his is the only conceivable one.

My argument on this verse contains three points. (1) The verse is not found in Pahl., Som., Kṣem., or Simpl., all of which H. assumes to be derived from "K", and therefore—being based only on n-w and Pūrṇ.—the verse offers no reliable evidence for "K". (2) It does not even offer reliable evidence for a hypothetical archetype of n-w and Pūrṇ. alone, since it is easily conceivable that Pūrṇ. got his readings for the verse from a source dependent on n-w or from a source outside the Pañc. circle. (The verse is of the sort which might be used anywhere at all.) (3) Most important of all: the reading of n-w and Pūrṇ. is more likely to be the Urpañc. reading than that of Tantr.

¹ The Hindus—rightly, it seems to me—treat such forms as *kar-madhārayas* (e. g. "irresolutely fearful"); Delbrück, AIS., p. 73, takes the same view, but the high authority of Wackernagel is on the other side (A-I. Gram. II. 1, Section 74 a).

If I am right in any one of these three contentions, then another of Hertel's "proofs" for "K" has been shown to be a mere subjective impression, and an improbable one.

(c) Another passage—and one which Hertel considers his most powerful argument for the "corrupt archetype K"—is discussed in his *Pañc.*, p. 440 ff. (cf. further *Tantr. Uebers.* I. 28 ff., 158). It concerns a supposed lacuna which he thinks he can establish in "K" in the story corresponding to *Tantr.* II. 2. The *Kathāsamgraha* or catch-verse to this fable reads in *Tantr.* thus:

*nākasmac Chāṇḍilī mātā vikrīṇāti tilāis tilān
luñcitāñl luñcitāir eva kāryam atra bhaviṣyati.*

That is: "Not without reason does Mother Čāṇḍilī offer to sell sesame for sesame, huskt for likewise huskt; there is surely something in that ('a nigger in the woodpile')".

In the prose text of the story, according to *Tantr.*, Čāṇḍilī sends a boy to exchange some huskt white sesame, which had become defiled, for black sesame, insisting that it should be likewise huskt. She thus hopes, by emphasizing the equality of the bargain, to escape the natural suspicion that there was something wrong with her sesame.

In most of the other recensions the catch-verse is changed in such a way as to eliminate the demand for huskt sesame in exchange for huskt. Only in one MS. of *Simpl.*, and in certain descendants of *Simpl.*, do the words *luñcitāñl luñcitāir* (or equivalent), "huskt for huskt", occur in the verse. I think Hertel is probably right in maintaining that this is the original reading of the verse; the other versions are then corrupted (or at least changed) at this point. Nevertheless they cannot go back to a corrupt archetype. "K", if there ever was a K, must have had the original reading (as in *Tantr.*) at this point; this is shown by *Simpl.*, and is admitted by Hertel. All the offshoots of K, then, except some texts of *Simpl.*, have become *independently* corrupted in this verse, according to Hertel himself.

But, says Hertel, all the "K" versions (except *Bṛh.*, cf. below) in the prose text of the story say that Čāṇḍilī tried to exchange her huskt sesame for *unhuskt*. This is true even of the one MS. of *Simpl.* which in its catch-verse speaks of

"huskt for huskt". In this MS., then, the prose is inconsistent with the verse; and in all the "K" versions except B_{rh}, the prose is inconsistent with the assumed original form of the verse (though not with the verse as read in these versions themselves).

So far I can find no fault with Hertel's reasoning. I agree with him that the evidence seems to indicate a departure from the original point of the story in most of the versions. And the departure is in all of them the same as to general sense, though not as to verbiage; this would scarcely be expected in the prose. I should further admit that if a *very large number* of cases could be found in which the so-called "K" versions similarly agree in readings which can be shown to be secondary or corrupt, as against the Tantr., then there would be reason for assuming Hertel's "K" archetype. But since this is the only case in which it seems to me that Hertel has made such a thing appear even plausible, and since equally or even more plausible cases could be made out for "corrupt archetypes" of any two Pañc. versions you could name (see below, p. 275 ff.), I do not admit that this single instance proves anything; nor would any two or three, or even ten or a dozen similar instances prove anything. For anything like proof we must expect cases which are either much more numerous, or much more compelling—preferably both. On the contrary, in view of all our present knowledge it seems to me much more likely that the change—or corruption, if you like—came in independently in all the "descendants of K". In any case, I think that there is no reason of weight for assuming that precisely a "lacuna" is indicated in "K" at this point, as Hertel does. On the contrary, as I shall show in a moment, I think that the occurrence of an identical proper name in both Tantr. and SP. at the place of the supposed lacuna shows that there was none.

That the same corruption could occur independently in many versions is admitted by Hertel, both for the catch-verse of this very fable, and in other connexions; thus in his Tantr. Uebers. II. 17, note I, he assumes for the original Pañc. a passage which is found *only in Tantr. and Hit.* Here then is a "lacuna" which has crept in independently in all of the "K" versions which have the assumed corruption in the

sesame story. Why then should H. assume that in the sesame story this precisely similar corruption (for so it is, according to his theory—a lacuna) proves a corrupt archetype?

His arguments for this assumption seem to me both subjective and flimsy, and beside the point. Thus, he says the story as told in Tantr. is much more clever; no one, he says, could possibly have been deceived by an offer to exchange huskt for unhuskt sesame. Even if this were admitted, it would have no bearing on the question. Whether stupid or clever, the alteration in the story must have been made at least once; why may it not have been made three times? Moreover I do not find the "K" form of the story so inconceivably stupid. A closely analogous motive occurs in the familiar story of Aladdin in the Arabian Nights ("Tausend und Eine Nacht"); the offer of "new lamps for old" worked successfully there. Evidently to the oriental mind, at least, this idea was not too stupid to pass in a story. It seems to me that there are in the Tantr. itself much more stupid and banal stories than the "K" version of this story. The latter seems to me, in fact, a more natural turn for the story to take than the almost *too* clever version of the Tantr. I can very easily understand the repeated change, in several (three) versions independently, from the very *recherché* to the more simple trick. The change, furthermore, may easily have been a mere verbal slip; it may have arisen simply from a mistaken assumption of an *a*-privative in *sandhi* (*luñcita* : *aluñcita*). We do not know what the original text of the Pañc. was at this point.

It should further be emphasized that both Som. and Kṣem. (descendants of "K", according to Hertel) say nothing about either huskt or unhuskt sesame. Their accounts of the story are, as usual, abbreviated. Som.'s version, in fact, is so brief that it does not bring out clearly the point of the story. Hertel's theory—more ingenious than probable—is that Som. still shows in his text the "lacuna" which he assumes for "K" at this point, and which he thinks has been "falsely restored" in the other versions. He does not say how he interprets Kṣem.'s version, which summarizes, though briefly, the part of the story omitted in Som., and which goes back to the same immediate original with Som.

But Som. and Kṣem. are frequently so abbreviated that

even important points of the story are obscured or omitted; this by no means indicates a lacuna in their original. SP. contains, in the part of the story omitted in Som. and therefore, apparently, in the supposed "lacuna" of "K", the proper name Kāmandaki, which is found at this point in Tantr. and could scarcely have been supplied from the imagination of the SP. redactor. This militates against the assumption of a lacuna between SP. and the common archetype of it and Tantr. And in general the versions of SP., Pahl. and Simpl. agree quite as closely as usual with the version of Tantr. throughout this story. The only variation of any consequence is the change of huskt to unhuskt; this, as I have indicated, may rest on a very slight verbal misunderstanding, and in any case much more serious variations than this occur repeatedly in all of these versions individually. That in this particular case the same corruption should have occurred in all three is indeed worth noting, but need not prove anything except that the corruption was a very easy and natural one. Even Hertel admits, as I have said, similar independent but identical changes for these versions at other points.

Recapitulating, my position is that Hertel's theory of the passage, while abstractly not impossible, is certainly not definitely proved—to put it very mildly. Another theory seems at least as liable to be correct (much more liable, in my opinion), namely, that the change of the word "huskt" to "unhuskt" in the prose story occurred independently in the texts (or the archetypes) of Pahl., n-w and Simpl.—exactly as, according to Hertel's own admission, this same change did occur independently in the catch-verse of the story.

(d) The only other case for the "archetype K" mentioned in Hertel Tantr. Uebers. I. 28 ff. is the verse Tantr. II. 25 (l. c., p. 32, note 1), in which he claims that all "K" versions read *sutaptam* wrongly for *ataptam* of Tantr. (A more lengthy discussion of this WZKM. 25. 13 ff.) The Tantr. stanza means: "With an enemy one should not ally himself, not even by a very intimate alliance. Water, even though not heated (or, punningly, "not injured"), puts out fire". The other versions, reading *su-* for *a-*, mean: "... Water, even if it be heated hot (i. e. even if its nature be changed so as to be as close to the nature of fire as possible), still puts out fire".

This seems to me to make as good sense, and to be as suitable to the context, as the Tantr. reading. Even if it is a secondary reading (which I am far from admitting), it depends on a very slight verbal change, which is furthermore suggested by the most natural meaning of the root *tap* in such a connexion. Water is very frequently "heated"; it is hard to conceive of its being "injured". It is only through the *double entente* in the word *a-taptam* that the Tantr. has any meaning at all. Therefore it would be easy to suppose that the corruption—if it be one—occurred independently in several versions, which failed to see the rather *recherché* meaning of the reading with *ataptam*, and thought they saw a chance to make the verse sensible. And in fact, the "K" reading is quite as good, and quite as apt to be the original Pañc. reading, as the other; in my opinion, more so.

(e) In WZKM. 25. 9 ff. Hertel adds another passage which he thinks points to a corrupt archetype "K". This is Tantr. I. 19, in which Tantr. reads *dhūrtam tam*, while the "K" versions have *dhunvantam*. The two readings are easily confused in the Çaradā alphabet, and the most natural supposition would be that the variant or corruption came in in the one version of the Pañc. whose MSS. are written in that alphabet—namely, Tantr. Of course, Hertel assumes the opposite.—He translates *dhūrtam* "verschlagen", "cunning", which must be taken (as the context shows) in a good sense, "clever". But *dhūrta* seems always to be used pejoratively; it means "a sly rascal, a cheat". This does not at all fit the verse; the reading *dhunvantam* does fit it.—Hertel (l. c., p. 11) refers to the reading *dhūnvantam* of some SP. MSS., and says that the "false" *ū* points to the *ū* of *dhūrtam*. The *ū* is not at all false; the root *dhū* is a well-recognized by-form of *dhū*, and the present *dhūnoti* is found both in the Veda and in Classical Sanskrit; see BR., and Whitney's "Roots, Verb-Forms", etc., s. v.¹

¹On p. 18 of WZKM. 25 I note what seems to me an astonishing remark from a scholar so familiar with the habits of Indian MSS. as Hertel is. "The reading of the majority of the MSS., FOEI, is perfectly comprehensible; therefore the reading of HM cannot be a correction of that of FOEI (!). Graphically, also, the reading of HM cannot be explained from that of FOEI. So there must have been

III. So much for Hertel's arguments about his archetype "K". On equally weak, or even weaker, grounds rests his assumption that Pahl., n-w (SP., v, Hit.) and Simpl. (with Pūrṇ.) go back to a nearer archetype, which he calls "N-W", a descendant of "K" of which Som. and Kṣem. are independent. So far as I can see, his reasons for this are just two. One is that in the sesame-story above mentioned (p. 266 ff.) those versions seem to him to have "restored" in a similar way the "lacuna" of "K", which Hertel thinks is still found in Som. This restoration, he says, must go back to an archetype of these versions. As I have already indicated, the lacuna and its restoration seem to me alike imaginary.

The only other attempt to prove the reality of "N-W" which I am able to find in Hertel's writings is that treated in Tantr. Uebers. I. 32 f. (cf. also SP. Einl. XXXVI ff.), the story of the brahman and the rogues (Tantr. III. 5). It is necessary here again to distinguish between what Hertel has a sound philological basis for asserting, and what he merely conjectures on purely subjective grounds; unfortunately he states both things with equal assurance. In the text of SP. (or at least in an archetype of all of Hertel's MSS. thereof except possibly T and X, both secondary MSS.) there was a lacuna in this story. I do not see how anyone could doubt this, after inspecting the table of the MS. readings on SP. Einl. XXXIX, and comparing the text, p. 47, with the critical notes thereto, p. 104. The point is almost superficially obvious; in a large group of MSS. the gap is still there. But in extending this lacuna back to a hypothetical common archetype of SP., Simpl. and Pahl. it seems to me that Hertel again

some special reason for the change . . .". A most amazing proposition, that there must be some "special reason" for every little variation introduced by a stupid, slovenly Hindu copyist. The change in question, for which Hertel thinks a "special reason" must be sought, is a shift between the syllables *tī* and *tya*. I am sure that Hertel knows as well as I do that much more serious changes occur constantly in Hindu MSS. without any "special reason" except the carelessness and ignorance of copyists. I only mention this remark—which I am sure Hertel would not defend in cold blood—to show how he is liable to be entirely carried away by the desire to prove a point, so that he momentarily fails to see the most commonplace things in their true light.

becomes subjective and wholly inconclusive. That Pahl. and the Jaina recensions speak of only three rogues,¹ while Tantr. and Som. speak of six (who however appear in three groups—first one, then two, then three), is surely no matter for surprise, and scarcely even calls for comment. In Tantr. and Som. first one rogue speaks to the brahman, then two, then three. The brahman is only addressed three times; why (say the later redactors) should there be more than three speakers? The climactic arrangement of the numbers is exactly the sort of trifling detail which we find later redactors constantly altering, either carelessly, or deliberately (because there seemed to be no reason for it). The only reason, indeed, which Hertel can think of for its being used in the original is that perhaps the author wanted to give examples of parallel Sanskrit forms in the singular, dual and plural. The passages as given in Pahl. and Simpl. are just as well and as fully told as in Tantr. Nor do they differ from Tantr. any more than usual, if as much. It seems scarcely believable that the trifling change about the numbers should seem to Hertel to indicate a “lacuna” in the archetype of these versions; yet such appears to be his argument. If there were marked variations—especially absurd or inconsistent ones—in the story, as there are in the individual MSS. of SP., whose text actually has a plain gap in many MSS., then indeed something of the sort might be assumed with more speciousness; although I have indicated above the danger of basing arguments on even such grounds.

I have tried to show that the arguments advanced by Hertel in support of his “K” and “N-W” are individually inconclusive. It seems to me that they are open to this more general criticism: the number of instances he adduces is too small to prove anything anyhow. Even if it were true that in these few

¹ In Old Syriac there are four; but the Arabic and its descendants make it clear that Pahl. had three. The slight variations in the number which appear in descendants of Pahl. show how absurd it is for Hertel to make so much of this infinitesimal point. Some of the descendants of the Arabic even have only two rogues. Yet we *know* that Syr., as well as Arab. and all its descendants, came from one single text—Pahl.—not from several “archetypes”—and that *that* text had some perfectly definite number here—whether four, three or two. Yet its descendants vary.

instances the Tantr. alone has the original reading (and there is only one instance in which there is good reason to assume this) and even if the other versions did presuppose an identical corruption, it is quite possible to believe (as I have already hinted) that these few changes crept in independently in the different "K" or "N-W" versions. My experience with the same problem—which confronted me in dealing with the interrelation of the versions of the Vikramacarita—leads me to the conviction that the problem is far from being so simple as H. seems to conceive it. Hindu literary tradition is such a terribly complicated matter, that in no work of the size of the Pañc. or the Vikramacarita could interrelationship be decided by any three or four, or half a dozen, cases of agreement or disagreement in detail—however striking. By such arguments the close connexion of any two different recensions of any Hindu work could be proven. When my "composite outline" of the four principal Vikr. recensions is published,¹ it will show at a glance the truth of my statement. This "composite outline" summarizes the entire work in all versions, and shows that each version agrees with every other version in a certain number of corruptions or changes, as well as in original readings. I can also show that the same holds good of the Pañc. versions. A few examples will be given in a moment.

But genetic relationship must rest on much broader considerations than this: on sweeping and extensive changes from the original plan of the work as a whole, or on extensive and far-reaching verbal agreements, or on a *very large* number of common corruptions or changes in detail. On such broad and sound considerations Hertel bases his statement, for instance, that SP., *ν* and Hit. are closely related, and that the latter two are closer to each other than to SP.; also, that Pūrṇ. used both Simpl. and a Tantr. codex for his new version. But up to this time Hertel has produced no proof of *this* sort for his archetypes "K" or "N-W". Until he does so he ought not,

¹ My two-volume work containing a complete text and translation of the main versions of Vikr. in horizontally parallel sections, with introduction, critical apparatus etc., has been in the hands of the printer for nearly two years; the delay has been in no way my fault, and has been much more deeply deplored by me than it could have been by anyone else.

it seems to me, to claim that these hypotheses represent anything more than his personal opinion and impression. As such they are certainly entitled to respectful consideration, although after a careful study of them I have been forced to disagree. But certainly they cannot properly be used as established facts in demolishing his critics (as Hertel does use them repeatedly; see for instance ZDMG. 68. 66).

Now in order to show that such cases as Hertel uses to prove his archetypes "K" and "N-W" may be found to establish archetypes for any two recensions—or even sub-recensions—of the Pañc., and thus to furnish a *reductio ad absurdum* of his argument, let me tabulate a few striking cases of variant readings, all taken from the verses of Book I of the Pañc., which I have carefully studied from Hertel's very useful tables of correspondences.¹ It will appear from these materials that, by picking out here and there a few verbal correspondences or common variations, such as Hertel uses to establish his "K", it would be possible to prove that Tantr. α and SP. α go back to a common archetype different from Tantr. β and SP. β, or that Tantr. and ν are similarly con-

¹ Tantr. Uebers. I. 100 ff., Pañc. 78 ff. I have discovered a number of errors (some of them, I presume, misprints) in these tables. In the first-mentioned correct as follows: *p.* 100, opposite Śār. I. 2, insert in Pa. column "Syr. Schulthess *p.* 1, l. 21 ff."; *p.* 104, opposite Śār. (Hertel's abbreviation for Tantr.) 65, read Pa. 57 for 55; opposite Śār. 128, insert Kṣem. 82 cd, 83 ab; below SP. 63 insert 64 (= Pūrṇ. 230) with dashes in all the other columns; Śār. 73 corresponds to SP. 66, ν 49, Pa. 51, and Som. 119 as well as to SP. 65 etc. (cf. my remarks below, *p.* 278); opposite Śār. 78 insert Pa. A 37 (?); *p.* 105, opposite Śār. 92, insert Pa. A 55, end; *p.* 106, opposite SP. 106, read Pa. 76 a instead of A 65; *p.* 107, opposite Śār. 126, read Pa. 81 instead of 79; *p.* 109, opposite Śār. 168, in Kṣem. column delete *ab*.—In the second-mentioned table (Pañc. 78 ff.) correct as follows: *p.* 78, read Śār. Kathāmukha 3 instead of 1 (opposite Pūrṇ. Simpl. 1); read Simpl. Kathāmukha 6 for 7; opposite Śār. I. 1 insert Simpl. 1; opposite Śār. 22 insert Simpl. 63; *p.* 79, the Simpl. references opposite Pūrṇ. 219 really correspond to Pūrṇ. 220 (which is omitted by mistake) and Simpl. II. 157 should be read for II. 160; Śār. 73 = Pūrṇ. 232 ab and 233 cd (cf. above, and *p.* 278 below); footnote 8, read 280 for 278; *p.* 80, below Pūrṇ. (Story) XIV, insert as follows: Śār. 120 = Pūrṇ. 310, Śār. 122 = Pūrṇ. 311 = Simpl. 308, Śār. 124 = Pūrṇ. 312 = Simpl. 309; also, below Śār. (Story) XIV insert as follows: Śār. 161 = Pūrṇ. 384 = Simpl. 394, Śār. 162-3 = Pūrṇ. 385-6.—Furthermore: Pūrṇ. 404 = 409, Simpl. 409 = 414.

nected, etc. Since of course we know that these conclusions would be unwarranted, I think it is thereby proved that Hertel's method of proving his archetypes "K" and "N-W" is unsafe and unscientific.

(a) Tantr. I. 5 = SP. I. 7 = ν II. 4 = Pa. I. 2 = Pūrṇ. I. 8 = Simpl. I. 21. (Correspondents in Brh. furnish no evidence on our point; neither does Pa.) In pada c, Tantr. text (with α) reads *sa naro nihataḥ ṣete*; SP. text, and ν , *sa bhūmāu nihataḥ ṣete*. The reading of Simpl. and Pūrṇ., *sa eva nidhanam yāti*, would seem at first sight like a secondary change. But looking at the variants of Tantr. and SP., we find that Tantr. β reads *sa naraḥ pralayam yāti*; and the MS. of SP. which Hertel considers on the whole the most original, viz. K, reads *sa eva nilayam yāti*, which looks like a cross between Tantr. β and Simpl.-Pūrṇ., while three MSS. of SP. β read exactly as Simpl.-Pūrṇ. Now try to construct archetypes, Hertel-fashion, for these readings, and what do we get? On the one hand—

Tantr. α :	<i>sa naro nihataḥ ṣete</i>
SP. (most MSS.) and ν :	<i>sa bhūmāu nihataḥ ṣete</i>

On the other hand—

SP. β 3 MSS., Simpl., Pūrṇ.:	<i>sa eva nidhanam yāti</i>
SP. α 1 old MS.:	<i>sa eva nilayam yāti</i>
Tantr. β :	<i>sa naraḥ pralayam yāti</i>

Apparently, therefore, Tantr. α , ν and most SP. MSS. go back to one "archetype", while Simpl., Pūrṇ., one SP. α MS. and 3 SP. β MSS. go back to another, which is also the principal basis of Tantr. β , though that has taken the word *naraḥ* from the other!

In particular, the ending *niyataḥ ṣete* is found in good descendants of both Hertel's "K" and his "Ś"; likewise, the ending *yāti* (for *ṣete*) preceded by an accusative. What becomes of the "archetypes K and Ś"? How did each of them read here? "Fortunate restoration" will hardly serve to explain such a change as this. It must be due to the floating and indefinite character of the Hindu literary tradition, which refuses to be run into theoretical molds to the extent that Hertel tries to do so.

(b) Tantr. I. 10 = SP. I. 13 = ν II. 9 = Simpl. I. 24. Pada d: Tantr. α and most SP. MSS. read *cirāya*; Tantr. β , Simpl., ν and two MSS. of SP. α read *ciraṃ ca* (one SP. α MS. *cireṇa*). What did "K" read?

(c) Tantr. I. 59 = SP. I. 50 = ν II. 36 = Pūrṇ. I. 159 = Simpl. I. 207 = Kṣem. I. 28 cd, 29 ab. Padas cd: Tantr. α and β , and SP. in all MSS. recorded by Hertel read *kāki* (SP. β *kākaḥ*) *kanakasūtreṇa kṛṣṇasarpaṃ amārayat*. But ν (the closest relative of SP.) reads, with Pūrṇ. and Simpl., in d *kṛṣṇasarpō nipātitaḥ* (in c Pūrṇ. and Simpl. read *kākyā*, ν *kākikanaka*^o). The reading of ν , Simpl. and Pūrṇ. seems to be confirmed by Kṣem., which according to Hertel used only Tantr. β besides its main source Brh. (a descendant of "K"); Kṣem. I. 29 ab reads *ṣrūyate kṛṣṇasarpō hi dhiyā kākyā nipātitaḥ*. What becomes of Hertel's archetype "K" at this point? Did it agree with ν , Simpl., Pūrṇ. and Kṣem., or with SP., the antiquity of whose reading is vouched for by Tantr.? Can Hertel possibly maintain that SP.'s reading is a "happy restoration"?!

(d) Tantr. I. 60 = SP. I. 51 = ν II. 37 = Pūrṇ. I. 165 = Simpl. I. 210. Pada c: all MSS. of SP. read *kaṣcin*; so likewise Simpl. and Pūrṇ., apparently establishing the reading of "K". But ν reads *paṣcān*, with Tantr. Apparently, then, ν would belong to "Ś", the archetype of Tantr. Or is this again a "fortunate restoration"?

(e) Tantr. I. 64 = SP. I. 56 = ν II. 41 = Pūrṇ. I. 221. In pada a: Tantr. α and SP. α read *abhyucchrite*, Tantr. β , SP. β and Pūrṇ. *atyucchrite*. It would appear, then, that Tantr. α and SP. α form one archetype, as against Tantr. β and SP. β .

(f) Tantr. I. 81 = SP. I. 73 = ν II. 54 = Pūrṇ. I. 251. Pada a: Tantr. α has *kuṣālān*, β *kuṣalo* (Hertel's text '*kuṣālān*'); SP. Pūrṇ. *vyasanān* (so establishing "K"'s reading), but ν *kuṣālā* (with Tantr.!).

(g) Tantr. I. 105 = SP. I. 92 = ν II. 71 = Pūrṇ. I. 245. Pada b: SP. β , ν and Pūrṇ. read *kamalaropaṇam*; SP. α with Tantr. '*bjam avarop*^o. What did "K" read?—The last parts of padas b and c, as read in Tantr. and SP., are transposed in ν and in Pūrṇ. What was their order in "K"?

(h) Tantr. I. 153 = SP. I. 132 = ν II. 106 = Pūrṇ. I. 373. Pada a: Tantr. α , SP. α and ν read *kaluṣeṇa*; Tantr. β , SP. β

and Pūrṇ. *kaṣaṭena*. (Hertel follows Tantr. *α* in his text of Tantr., but SP. *β* in his text of SP.)

(i) Tantr. I. 155 = SP. I. 133 = *ν* II. 107 = Pūrṇ. I. 375. Pada c: Tantr. *α* with *ν* *ṣaṣāṅkasya*; Tantr. *β* with SP. and Pūrṇ. *saṣaṅkasya*. Hertel thinks the reading of Tantr. *α* is the original, and that of *ν* a "fortunate correction". The meaning of the verse is dubious; I am not satisfied with Hertel's interpretation.

(k) Tantr. I. 158 = SP. I. 136 = *ν* II. 109 = Pūrṇ. I. 381. Pada b: SP. and Pūrṇ. have *tam anuv°*, Tantr. and *ν* *samanuv°*.—Pada d: Tantr. Pūrṇ. *bhavaty āmalakīphalam*. The reading which would be indicated for "n-w", by the general consensus of the SP. *α* MSS. and *ν*, is *jāyate* (or with *ν* *jāyetām*) *kaṇṭakam phalam*. Nevertheless SP. *β* (a secondary recension) reads exactly as Tantr. Pūrṇ., except *āmalakam* for *°kī*. Apparently SP. *β*, Tantr. and Pūrṇ. go back to a different "archetype" from SP. *α* and *ν*. The variants are of a sort which could not possibly depend on a "fortunate restoration".

(l) Tantr. I. 169 = SP. I. 143 = *ν* II. 116 = Pa. I. 103 = Pūrṇ. I. 396. Pada c: SP. *β* alone reads *sneho*, "affection", for *guhyaṁ*, "a secret", of all other Sanskrit versions, including SP. *α* and *ν*. But Pa. translates a Sanskrit word which must have been *sneho* or a synonym thereof, and cannot have been *guhyaṁ*: "die Freundschaft bis zu den Verleumdern" (Schulthess). Apparently, then, SP. *β* and Pa. go back to one "archetype", while SP. *α*, *ν*, Tantr., etc., go back to another.

(m) Tantr. I. 174 = SP. I. 145 = *ν* II. 118 = Pa. I. 106 = Pūrṇ. I. 403. (Cf. Hertel SP. LVI f.) In pada c: Tantr. *α* and apparently (so Hertel) SP.'s archetype (with MS. K) read *anugamyō* (most SP. MSS. have unimportant corruptions). But Tantr. *β* and Pūrṇ. have *anukampyo*, to which the corrupt text of *ν* (*—kam̐o—*) evidently goes back. Here then we have Tantr. *β*, Pūrṇ. and *ν* forming one group, Tantr. *α* and SP. another.—The lengths to which Hertel allows his preconceived theories to carry him are indicated by his discussion of pada b (l. c., LVII). Here nearly all MSS. of SP., including K, the best MS. of all, and B, another MS. of the *α* recension, read *bhāvyam*, which the meter requires.

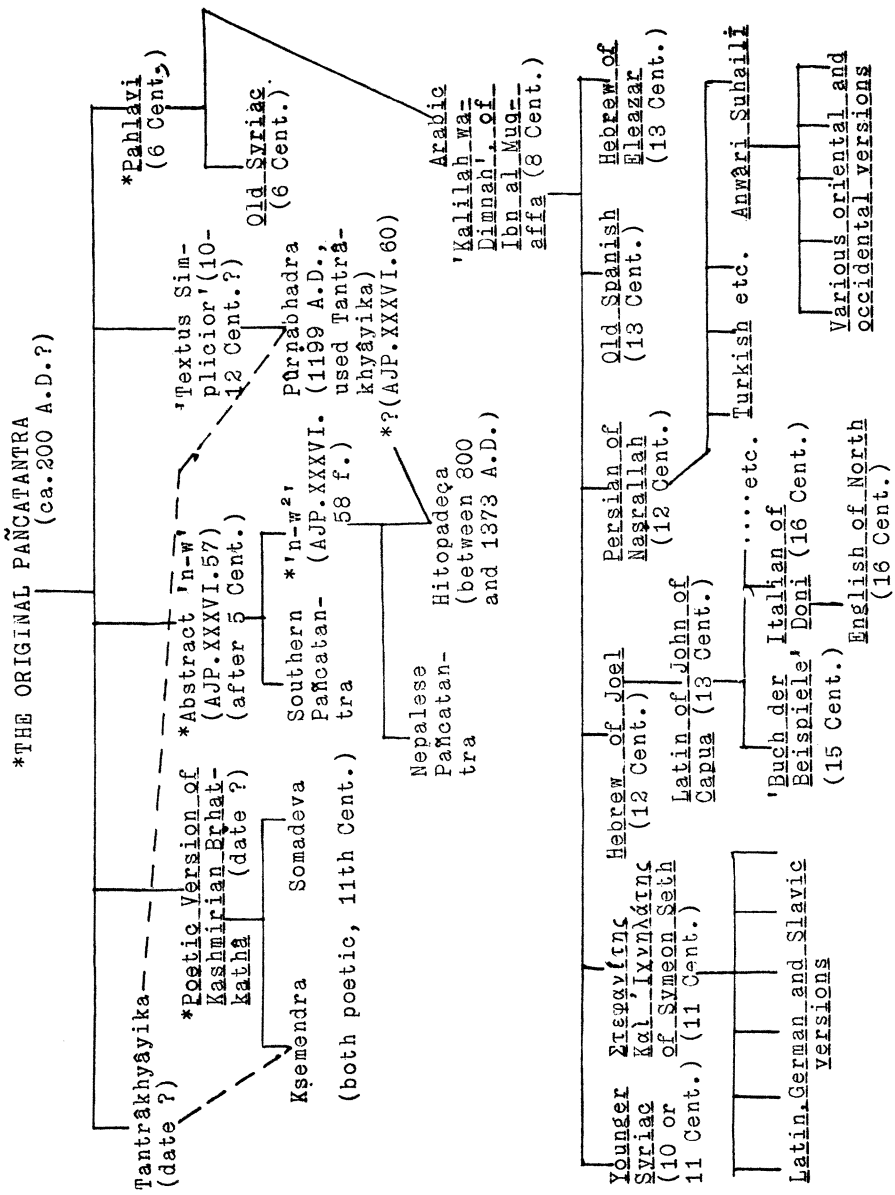
A few *a* MSS. read *bhavitavyam*, an obvious and metrically impossible corruption. Hertel finds the evidence of these few corrupt MSS. enough to *absolutely establish* the corruption *bhavitavyam* for the SP. archetype—apparently for no other reason than that the Tantr. MSS. have the same corruption, and Hertel feels the need of making SP. at least as corrupt as the Tantr.!

I have chosen only the most clear and striking cases of this sort which I have discovered in the verses of Book I. The same sort of thing occurs repeatedly in smaller variations which could more easily have arisen independently; that is why I have not recorded them—although they include a considerable number of much more marked and important variations than some on which Hertel lays great weight, and in which he sees a deep significance (cf. WZKM. 25. 16 ff., and my comments, above, p. 270 n. 1).

(In Tantr. I. 73 is a clear corruption which Hertel has failed to perceive. It contains fragments of two verses found entire in the other recensions—SP., *v*, Pa., Pūrṇ. and even Som.; SP. I. 65 = *v* II. 48 = Pa. I. 50 = Pūrṇ. I. 232, clearly reproduced by Som. 60. 119, while SP. I. 66 = *v* II. 49 = Pa. I. 51 = Pūrṇ. I. 233, clearly reproduced by Som. 60. 121. Now Tantr. I. 73 ab is SP. I. 65 ab, while Tantr. I. 73 cd is SP. I. 66 cd. Padas cd of SP. I. 65 and ab of SP. I. 66 are accidentally omitted in Tantr. Correct Hertel's statement under Tantr. I. 73 on p. 104 of Tantr. Uebers. I.)

I hope to have shown that there is a total lack of definite proof for Hertel's archetypes "K" and "N-W". On the contrary, as I have hinted, it seems to me that Pahl., Brh., Simpl. and n-w are each more closely related to Tantr. than to each other. At present I do not think it is possible to determine more narrowly the relationship of these versions. But in determining the Urpañcatantra version at any one particular point, I hold—in direct opposition to Hertel, whose views rest on the assumed stability of his "K" and "N-W" archetypes—that the agreement of any two or three of the older versions establishes a *prima facie* probability that they contain the original reading—*provided* that the corresponding texts of the other recensions show no agreement among them-

GENEALOGICAL TABLE OF THE OLDER AND MORE IMPORTANT
VERSIONS OF THE PAÑCATANTRA.



* Indicates non-extant versions.
— indicates influence in minor degree.
Translations into other languages than Sanskrit are *underscored*.

selves. *Nota bene*: a *prima facie* probability only; and *only* in case the disagreeing versions are divergent among themselves. Many of Hertel's reconstructions of the Urpañc. are vitiated by his unwarranted assumption that the evidence of Tantr. alone is of at least equal weight with the united evidence of *all* the other versions; nay, he sometimes thinks Tantr. α alone can outweigh Tantr. β with all the other versions, since he believes that Tantr. β is contaminated from a "K" codex. I believe that Tantr. α and β together are of not much, if any, more weight in determining the original Pañcatantra than any other of the older versions.

My opinion of the relation of the older Pañcatantra versions is indicated in tabular form by the following "genealogical table", which I would substitute for Hertel's "Stammbaum" in so far as it is inconsistent therewith. Of course it should be understood that my table is only tentative. Especially is this true on the negative side. That is, I regard it as quite possible that closer relations may yet be proved to exist between some versions, between which the table indicates no closer relation, simply because no such relation has yet been *proved*. The relations indicated positively by my table I regard as so well established that it is unlikely that future investigations will upset them.

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